This paper proposes a study of the oceanic circulation in the Golf of Cadiz based on 5 years observation of surface currents (2016-2020) with 4 stations of High-Frequency Radars (HFR) deployed along the southern coast of Portugal. The combined radar stations cover an area of about 200 km along shore and 100 km offshore. The HFR currents are compared and validated with a series of in situ measurements including 3 ADCP moorings and 3 drifters.

The analysis is sound and well written. In the end it leads to a complete description of the main circulation pattern and its seasonal variations. I have no core objection to the main results and will only point out weaknesses in the methodology and possible improvements. Also, as an expert of HFR processing rather than oceanography, I will essentially comment on the techniques that are used to extract and validate the HFR surface currents.

Main comments

1) In the Abstract it is claimed that the analysis is made from « validated hourly HFR measurements ». However, it seems that the validation and comparisons with in situ measurements is made only with low-pass filtered data corresponding roughly to a daily average (40 hours Butterworth filter). This needs some clarification. In particular, i the EOF decomposition obtained from hourly data or low-pass filtered data ? Is the interpolation of small gaps (< 6 hours) made within the EOF process or is a preliminary ad hoc filtering ? This could lead to different outcomes.

2) As it is well known (Stewart and Joy 1974) , the HFR measurement integrates the current over a depth equal to a fraction of the radar wavelength (lambda/8 pi). This make the comparison with ADCP meaningful only if the depth of the measurement cell is comparable. I could not figure out from the manuscript the exact depth of the last bin in the various ADCPs. Could this be stated explicitely and commented ? A difference of measurement depth between the ADCP could account for part of the difference of performances in the HFR comparison. This information could be given or recalled in Table 1.

3) Figure 4 shows the radial current on a very coarse temporal scale. It seems that during the second half of january 2017 the HFR and ADCP current have significant difference (> 20 cm/s). Is there a reason or proposed explanation for this particular period ? Could subsurface processes and current shears be responsible for this (in relation to the former point regarding the ADCP measurement depth) ?

4) The RMSD between HFR and drifter measurements is very large (~ 25 cm/s in norm). Due to the motion of drifter I think the comparison with daily HFR currents is not very meaningful and should rather be done with hourly data. Furthermore, the drifters having no drogue, they are more sensitive to wind and near-surface current and therefore faster than the average current over the HFR integration depth (see for example Dumas et al., Ocean Dynamics 2020 for HFR comparisons with drifters with and without drogues). All in one, the drifters do not appear to be a relevant validation tool in this context.

5) Did you perform self-consistency tests to assess the validity and accuracy of the EOF reconstruction ? See for example Bourg & Molcard Ocean Dynamics 2021 for such kind of procedure.

Minor remarks :

- Line 133 p 5 : « ...is estimated from adjacent valid measurements ». I do not understand this statement. If the angle between radials is less than 20 degree, it will be more or less the same with adjacent measurements ?
- Line 134 p 5 : « The 2 references CODAR a,b seem to be incomplete. Are these tutorials, manuals, preprints ?
- The EOF method which is employed (Alvera 2005, Beckers and Rixen 2003) is today commonly referred to as « DINEOF ».
- At view of Fig 5 and Fig 6 on the EOF decomposition it seems than the mean field is included in Mode 1, which is 47 % of the variance. Can you please clarify whether Mode 1 in Fig 6 is a velocity anomaly or not ? Also please specify the scales and units in the plots. The amplitude of Mode 1 ranges from 0 to 20, this makes big values in the end when multiplying by the amplitudes of Mode 1 or 2.
- Regarding the phase ambiguity (180/-180) in Figure 7, this could be circumvented by plotting the cosinus or by unwrapping the phase.
- As noticed by the authors the phase of the spatial modes is close to 0 or ± 180 degree in Figure 7. Is this a criterion of correctness for the EOF decomposition ? Otherwise can one expect arbitrary values for this phase ?